At an entertainment, however, given when the first of the Conway tubes was completed, as our readers may be aware, Mr. Robert Stephenson, to whom Mr. Fairbairn attributed the original idea, had himself claimed everything, in short, but 'aid.' This occurred on 17th May, 1848.

"It is now upwards of six, or about seven years," said he on that occasion, "since I entertained the idea of constructing bridges with wrought-iron plates riveted together. I was called upon,—in a smaller case I admit, but not a very simple one,—to construct a bridge authorised by Act of Parliament, but with such limitations that it became a matter of extreme difficulty. All the ordinary kind of bridges were discussed, and I eventually hit upon the notion, and the designs were completed, *for a thin tubular bridge*, although not precisely the same as the present, yet *in principle precisely the same*. That was effectually completed, and answers its purpose, and may be now seen on the Northern and Eastern Railway. From that time, however, to the period of commencing the Chester and Holyhead Railway, the idea fell, or dropped rather, for the time, in consequence of the expense of wrought-iron rather exceeding that of cast. * * * * Parliamentary powers were granted for the construction of a bridge over the Britannia rock, with such conditions attached to it as rendered it all but, if not absolutely, impracticable. It was then, to use a common expression, that I felt myself fairly 'driven into a corner.' No existing species of bridge was at all applicable under the operation of the Act of Parliament as granted; and it was after an anxious investigation of every possible description of bridge, that it occurred to me, that by *reviving the old notion of seven years ago*, that by extending it, it might enable me to get over the difficulty. Approximate calculations were immediately made, and the result of those calculations were such as to satisfy me of the perfect feasibility of the work. I had satisfied myself that the thing was practicable, and I stood by it. I obtained the consent of the directors to institute a very laborious, and elaborate, and expensive series of experiments, in order, most thoroughly, to test experimentally the theory I had formed, and also to add suggestions for its full development. It was then that I called in the aid of two gentlemen, eminent, both of them, in their profession, Mr. Fairbairn and Mr. Hodgkinson. [Here follow a few compliments, of no importance to the point at issue.] But having mentioned these two names, there is another gentleman that I wish to call to your notice—I allude to my assistant, Mr. Edmund Clarke. He has been my closet companion from the commencement of the preliminary investigation; no variation or inconsistency in the experiments eluded his keen perception; he was always on the look out for contingences that might affect the success—though not the principle, still the success—of the undertaking; and he and the other gentlemen whom I have just named, are the three to whom I feel deeply indebted for having brought the theory I first broached to such perfection, and I thus publicly tender them my acknowledgments."

Mr. Fairbairn more particularly alludes to this speech in his concluding remarks. Our quotation of these, however, like that of the preceding, must be a condensed and brief one.

"The inaccuracies, both as to facts and dates, in this statement of Mr. Stephenson," he observes, "are very numerous. It simply requires a reference to the short description of the Ware-bridge, p. 113, and to the drawings, to disprove the assertion, 'that it is a *thin tubular bridge*, although not precisely the same as the present, *yet in principle precisely the same*;' and it can be easily shown too, that considering the Ware-bridge as a simple girder bridge, it is exceedingly defective in design. Is there any thing new in this application of wrought-iron plate girders? As well might it be said that the combination of wrought-iron deck beams, so many years applied in iron ships for the support of the decks, is 'a counterpart of the proposed cellular top for the Britannia tubes.' I really cannot but regret that Mr. Stephenson, whose name will be always associated with the grandest bridge that has ever been constructed, should have committed himself in making such an erroneous assertion, as that it was by reviving and extending his original conception of this imperfect structure at Ware, that he was led to originate the bridges crossing the Conway and Menai Straits. * *

Let the reader turn again to the earlier letters of the correspondence, and he will find of what a crude and dangerous scheme that idea consisted; how totally dissimilar, in form and principle, it was to the present tubular structures, and how slowly Mr. Stephenson was persuaded to give up his earliest conceptions. Again, it was I, and not Mr. Stephenson, that solicited Mr. Hodgkinson's co-operation, and this was not done until I had been actively engaged for several months in my experimental researches, and after I had discovered the principle of strength which was offered in the cellular top, and not only proved the impracticability of Mr. Stephenson's original conception, but had given the outline of that form of tube which was ultimately carried into execution.

Mr. Bateman, C.E., showed the unjustifiable position which had been taken by Mr. Stephenson, and asserted, that in an engineering work of such novelty and magnitude, Mr. Stephenson would not have injured his own reputation, by acknowledging, in suitable and truthful terms, the merits due to those who had rendered him the most valuable service. Mr. Stephenson replied to his letter, and the tenor of his remarks showed his determination to stand by his public assertion. He quoted, from my letter of the 27th of October, 1846, my testimony to his claim of originality in having the application of *a* wrought-iron tube *for* the purposes of railway traffic; a great merit which neither Mr. Bateman nor myself had ever denied to him, and which I have uniformly asserted that he is undoubtedly entitled to. But he left entirely untouched the point at issue, viz., that it was almost exclusively my exertions which gave to his conception a useful and practicable form—that the experiments which I had conducted and originated, showed the weakness of the *circular* tube, which he had originally recommended—that I alone showed him the danger of the principle which he was anxious, for so great a length of time, to carry out, by attaching a flexible catenary to a perfectly rigid platform or roadway,—that from the results of these experiments, I designed and submitted for his approval an entirely novel kind of tubular bridge, different in form, different in principle, vastly superior in economy of material and strength, and which was finally approved and carried out, and which is now in existence, spanning the rapid estuary at Conway, and admirably fulfilling the varied requirements of railway traffic with perfect security."

In support of his allegations, Mr. Fairbairn adduces an abundant mass of correspondence and experiments, showing the gradual progression through which the series of trials naturally advanced from the first crude idea towards maturity. From this evidence it first of all appears that the idea—the original idea which he was called upon to test by experiment, or which at least *was* so tested, at Mr. Stephenson's instance,—was nothing cellular at all,—nay, not even modified as yet into *any* sort of scheme for strengthening either top or bottom, but simply a cylindrical tube made of riveted iron plates; and tubes such as these *altogether failed* to support anything like a safe or sufficient weight, uniformly yielding, primarily above to compression in wrinkles, cross corrugations, or puckers, but also readily to tension below, in disjointings and rents. These results were next followed by obvious appliances for the *strengthening, and even the local patching*, of those parts, *above at least*, which so readily gave way. In the midst of these we find Mr. Fairbairn's first suggestion of a form of tube not cylindrical and self-supporting, but intended, as he says, to meet Mr. Stephenson's views, though, as he thought, superior to one supported by chains, the necessity for which the latter appears to have urged even after he had been convinced by Mr. Fairbairn that the tube *would* be entirely self-supporting, as he still did not think it could be *put up* without the aid of that costly adjunct.*

The suggestion just referred to was made in a letter to Mr. Stephenson, dated 31st May, 1845, and enclosing a sketch of such a tube, strengthened above, but without the most distant idea or semblance of cells, as yet, even on Mr. Fairbairn's part. On the 3rd June following, a very ingenious suggestion is made by the latter for the erection of such a tube without even the temporary aid of the objectionable chains, by working simultaneously from each pier in opposite directions, so as to maintain a sort of equilibrium till the centres met. These and subsequent communications are followed up by a note from Mr. Stephenson, of date 21st July (1845), in which he says, with reference to the tubes, "*I hope* some of them, of an *elliptical* form, and *with thick plates* at the top and bottom, *will be tried*." Still nothing of the *principle*—the cellular—on the part of either. It is fair, indeed, to conclude, from Mr. Stephenson's own words, that as yet that principle had never even been dreamed of either by one or other of them.

"The difficulties," here remarks Mr. Fairbairn, "experienced in retaining *the cylindrical* tubes in shape, when submitted to severe strains, naturally suggested the rectangular form. Many new models of this kind were prepared and experimented on before the end of July, and others, with different thicknesses of top and bottom plates, or flanches, before 6th August." Of that date a letter, containing the following passage, was written by Mr. Fairbairn to Mr. Stephenson:—

"In almost every instance we have found the resistance opposed to compression the weakest; the upper side generally giving way from the severity of the strain in that direction. These facts are important so far as they have given rise to a new series of experiments, calculated to stiffen or render more rigid the upper part of the tube, as well as to equalise the strain, which in our present construction is evidently too great for the resisting forces of compression."

And now comes the germ of the cellular principle. "It occurred to me," says Mr. Fairbairn, "that the top might be strengthened by other means than by *thickening* it, and I addressed the following letter to my son, four days after the date of the last." In this he directs him to have prepared *a rectangular tube with a hollow triangular 'fin' or corrugation* (or cell, in fact) of sheet-iron, running along the top, to which it was to be riveted. The idea of a single long corrugation, to counteract the cross corrugation of the puckerings of compression, forthwith led him to that of two such corrugations doubled and reversed, so as to appear in *section* like a pair of spectacles; the superior strength of which, Mr. Fairbairn remarks, induced him "to adopt that cellular structure of the top of the tube which ultimately merged in a single row of rectangular cells—that structure which gives to the bridges now standing across the Conway straits their principal element of strength."

The next letter is one from Mr. Eaton Hodgkinson to Mr. Fairbairn, of date 18th of same month, written from the "British Iron Works, Abersychan, near Pont-y-pool," intimating that he was now at liberty to offer his services as desired by Mr. Fairbairn, who now found that "the experiments had assumed a shape which seemed to require the assistance of a mathematician, who should deduce, if that were possible, a formula which, from the observed strength of a tube of a lesser, might enable me to calculate the strength of one of a greater size." Mr. Hodgkinson accordingly first appears on the field of operations at Millwall on 19th September, the day when "the tubes which had been constructed with single hollow or cellular tops were experimented on."

While intimating the result to Mr. Stephenson, in a letter dated 20th September, Mr. Fairbairn says,—"You will be aware, on referring to my last letter, that the great difficulty we had to encounter was a due proportion of the parts, so as to neutralise or render the two resisting forces of compression and extension equal;" and, after reviewing the whole series of previous experiments, and pointing out the defects in cylindrical and elliptical tubes, even though protected by single 'fins' or corrugations, he goes on to say—

"It is more than probable that the bridge, in its full size, may take something of the following sectional shape. The parts, *a*, *a*, being two longitudinal plates, divided by vertical plates so as to form squares, calculated to resist the crushing strain in the first instance, and the lower parts, *b*, *b*, also longitudinal plates, well-connected with rivetted joints, and of considerable thickness to resist the tensible strain in the second."

In short, by a natural process somewhat the reverse of that whereby, according to the French anatomists, a 'cell' becomes a 'fin' as well as any other organ, Mr. Fairbairn's 'fin' had now, *and only* now it appears, assumed that very 'cellular' and rectangular form which he afterwards realised at the Conway Strait.

"It is from this period," he remarks, in allusion to these and other experiments immediately following on these, "that I date the disappearance of almost every difficulty respecting the construction and ultimate formation of the Britannia and Conway tubes."

When these experiments — but a faint glimpse of the numerous series of which only we have here had space to offer—were sufficiently matured, the importance of Mr. Fairbairn's *position* appears to have been such as to induce the directors of the Chester and

* Mr. Fairbairn estimated the expense of such a chain at about 200,000*l.*